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still is that we are unable to say for certain whether doubles invariably occur when the above-named species are crossed with the pollen of a double. When this evidence is available we may expect it to throw further light on the question as to which character is dominant. At present decisive proof on this point is lacking. Comparison with the other types carries us no further. Singleness has been found to be recessive in carnation, hollyhock and *Meconopsis cambrica;* on the other hand,—it is dominant in wallflower and probably in sweet william.[13] Moreover, these forms differ from *Petunia* in that they give a *uniform* $F_1$ when single and double are mated together. The case of *Petunia* therefore still remains one of balance of probabilities. In regard to Frost's further suggestion that the facts observed are due to selective sterility I think that this hypothesis may very possibly be correct and certainly has some evidence in its favor. *P. vidacea* is recognized as a self-sterile species and many of the singles which I used proved to be so. A large number of individuals were tested for this character, but further investigation was postponed in the hope of obtaining pure material for comparison. If Frost's hypothesis is confirmed for *Petunia,* and the work of Belling[14] and East[15] points in this direction, it may offer a complete explanation of the facts and render the supposition of a differential distribution of factors to ovules and pollen in this genus unnecessary.

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## GAMETOGENESIS IN PLANTS

THE evolutionary origin of the reproductive cells furnishes one of the most fundamental problems connected with genetics, for upon a clear understanding of the subject depends the satisfactory solution of many subsidiary problems relating to animal and plant breeding. The value of hybridization and inbreeding; the meaning of the pure line hypothesis; the principle of cumulability, etc., may here be mentioned. Therefore, whether or not one agrees with the conclusions presented, studies from widely divergent standpoints which bear upon the question are to be welcomed. It is only through an analysis of the opinions thus advanced that there will develop a perspective which will eventually permit the solution of the problem.

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[13] Saunders (unpublished). The carnation has also been investigated by Norton. (See paper read at the meeting of the Society of Horticulture in Philadelphia in December, 1904. Also *Gard. Chron.*, Jan., 1905.)

[14] *Loc. cit.*

[15] *Loc. cit.*

It is in this connection that the conclusions of Professor Coulter as set forth in "The Evolution of Sex in Plants"[1] are of interest, representing as they do the views of one whose attainments in biology have by no means been confined to the field of plant morphology. Presented in a clear and interesting manner so far as the facts are concerned, the volume furnishes a valuable résumé of the subject from the botanical standpoint. It is evident, however, that a certain narrowness must exist in such a presentation, for a problem of this nature demands that plant biology and animal biology supplement one another from the experimental as well as from the morphological and cytological side. Gametogenesis had its beginning not, as Coulter suggests, among organisms far above the most primitive plants, but among unicellular flagellate forms whose representatives partake of the nature of both plants and animals and from which have arisen the various groups of plants in general. Sexuality, once having arisen, may have been partially or even wholly suppressed in various plant groups, but its subsequent reappearance by no means makes it necessary to affirm its polyphyletic origin. Our present knowledge of Mendelian behavior is of interest in connection with such a view.

It will be well to examine some of the more definite conclusions which Coulter has presented. Few of these are original, nevertheless they are of decided value since they are in most cases supported by unique observations bearing directly upon the point of view. It is merely unfortunate that the bibliographic references which would illustrate the development of the ideas are entirely absent, in consequence of which a false impression may be conveyed to many readers.

Early in the volume it is stated that sex in the higher animals has become the only method of reproduction. Logically this view is not to be maintained, as has already been pointed out by LeDantec ('03) as well as by Chamberlain ('05) evidently in ignorance of the conclusions reached by the previous writer. More recently Janet ('12) has considered the subject. If the criterion by which the sporophyte is to be distinguished from the gametophyte rests upon the $2x$ as compared with the $x$ condition of the chromosomes, we find that among animal organisms the asexual phase has actually become the dominant method of reproduction and the sexual phase is represented only by the parasitic cells arising through the reduction division. In accordance with this view one is prepared to accept the spore mother cells of plants as homologues of the cells preceding the reduction division in animals. In a subsequent discussion Coulter states in accordance with the view first advocated that the animal body produces gametes and not spores. When reduction occurs at the time of the first maturation division in animal organisms it is quite clear that the cells thus produced may correspond to spores which in the next division give rise to gametes. It may even be asserted that they are megaspores or microspores dependent on the sex represented. When chromosome reduction is moved forward to the second maturation division, however, it is possible to agree with Coulter, but seemingly more logical to admit that the change is a secondary one and that the first maturation cells may still represent the spore cells.

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[1] By John Merle Coulter, head of the department of botany, Univ. of Chicago. Univ. of Chicago Press, December, 1914.

In accordance with the proposition that spores unite as gametes to form a single cell, evidence should either be presented to show that an identical chromosome composition exists between the actual spores and the so-called spores functioning as gametes or consideration should be given to subsequent reduction division. Otherwise the conclusion scarcely merits the value of an opinion. Furthermore, the argument that the fusion of a sperm and cell among the angiosperms to form a nutritive endosperm justifies the conclusion that pairing and fusing do not represent the essential features of sexuality, can not be considered. This is only one of numerous examples where changes in form or function of parts occur without having any bearing on the actual origin of the part. Even in this case a fusion is represented and may have a value similar to that among gametes.

It is in connection with "A Theory of Sex" that it seems necessary to decidedly differ from Professor Coulter. Here the two main theses are that sexuality has arisen (1) to carry an organism through an unfavorable environment, and (2) to make evolution more rapid by presenting a greater diversity of forms.

The first deduction is based on the proposition that gametes in many plants are produced at the close of the vegetative period. Such a conclusion—*post hoc ergo propter hoc*—does not rest upon a sound basis. With the fulfilment of a function having the importance of a gamete production, it is quite logical that the cycle of development should close, but to state that the closing of the cycle has brought about the production of the gametes, is quite another thing. The acceptance of this would lead to the inference that gametes arose in fresh or brackish water forms where pronounced seasonal changes took place and not in larger bodies of water like the ocean, the most probable place.

The second deduction is a restatement of a conclusion reached by Weismann ('76) to the effect that amphimixis increases variability with the assumption that variations thus assumed to be produced are inherited in a cumulative manner. The evidence, however, available at present, supports a view directly contrary to this, namely that the gametic condition makes evolution slower by decreasing the diversity of available forms. Mendelian combinations—amphimutations—may occur but the result is a decrease in variability when the parental populations are compared with the $F_2$ or with a succeeding generation. The amphimutations are transitory and there is no evidence that they present anything actually new in themselves.

Regardless of the opinions here at variance some of which can only be established as sound generalizations through long experimental investigation, the summary of gametogenesis by Professor Coulter will be read with profit and pleasure by those interested in problems of evolution as well as by those particularly concerned with plant morphology and development.

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